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ABSTRACT

Individual marital therapy (IMT) is therapy in which one or both spouses are treated separately by different therapists who do not communicate with each other about treatment. Gurman and Kniskern (1978) reported an improvement rate of 48% and a deterioration rate of 11.6% for nonbehavioral IMT. These outcome data and the conclusions drawn from them have precipitated oppositional reappraisals and critiques, notably by Jacobson (1978) and by Wells and Gianetti (1986). Jacobson disregarded Gurman and Kniskerns' outcome research on grounds of its generally inferior research design and methodology. Wells and Gianetti reappraised each of the original studies by Gurman and Kniskern and concluded that the studies had major flaws of design and implementation. This controversy may be resolved by: (1) examining reported efficacy rates for nonbehavioral IMT; (2) describing evaluations and interpretaions of the data; (3) explaining why alternative interpretations and explanations are given for the same outcome research; and (4) presenting an original critique and evaluation of the debate on IMT outcome research. Completion of these four steps suggests that the 1978 appraisals by Gurman and Kniskern of the improvement and deterioration rates for nonbehavioral IMT were based on studies which do not meet minimum requirements for outcome research. To date, there is little acceptable empirical evidence either for or against the efficacy of IMT. The exploration and generation of investigative questions on this issue, however, should be seen as a valuable and worthwhile endeavor. A seven-page list of references concludes the paper. (NB)

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THE EFFICACY OF NONBEHAVIORAL INDIVIDUAL MARITAL THERAPY

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Doctoral Research Paper

for the Degree of Doctor of Psychology

ROSEMEAD SCHOOL OF PSYCHOLOGY

BIOLA UNIVERSITY

THOMAS LEE MORSCHECK

1986

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THE EFFICACY OF NONBEHAVIORAL INDIVIDUAL MARITAL THERAPY

BY

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A doctoral research paper submitted to
Rosemead School of Psychology
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1986

THE EFFICACY OF NONBEHAVIORAL

INDIVIDUAL MARITAL THERAPY

BY

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TABLE OF CONTENTS

	Page
Abstract	v
Introduction	1
Reported Outcome Data for Nonbehavioral Individual Marital Therapy	5
Alternative Evaluations of the Outcome Research on Individual Marital Therapy	8
Gurman and Kniskern	8
Jacobson	10
Wells and Gianetti	12
Explaining Alternative Explanations According to Personal Characteristics	13
The Gadfly Characteristic	14
Forensic Skill	16
Explaining and Critiquing the Diverse Evaluations According to Methodological and Conceptual Themes	17
Theoretical Orientation	17
Internal versus External Validity	20
Minimum Criteria for Individual Marital Therapy Outcome Research	22
Goals of Research	30
Appraising the Appraisals: The Value of the Individual Marital Therapy Outcome Research	33
Research Idealism versus Clinical Realism	34
Inside versus Outside the Profession	34
Publication versus Policy	35

Principle versus Practice 36
Evaluative versus Scientific Research 36
Summary. 38
Bibliography 40

ABSTRACT

The efficacy of nonbehavioral individual marital therapy (IMT) is addressed via an examination and critique of the original benchmark reviews of Gurman and Kniskern (1975, 1978a, 1978b). A current reappraisal of the outcome research centered around the spirited debate among Gurman and Kniskern (1978b, 1986), Jacobson (1978), and Wells and Gianetti (1986) is presented. The debate among these writers has included disagreements regarding accurate improvement and deterioration rates for nonbehavioral individual marital therapy and the value of studies with major flaws in research design and methodology. The reappraisal reveals the inconclusive empirical evidence for or against the effectiveness of IMT, but nevertheless, the present author argues that judgements of the value of the outcome research are influenced by the goals and clinical orientations of the researchers.

THE EFFICACY OF NONBEHAVIORAL INDIVIDUAL MARITAL THERAPY

Introduction

The emergence of particular approaches to marital therapy, such as individual marital therapy (IMT), has fostered questions regarding efficacy of treatment. Efficacy refers to the changes and outcomes clients experience as a result of marital therapy. Typically efficacy is operationally defined according to the categories of "improved," "no change," and "worse."

Reviews of outcome research on marital therapy have been conducted to answer questions about treatment efficacy. The reviews by Gurman (1973b, 1975) and Gurman and Kniskern (1978a, 1978b) are specific examples. A controversial element in the above reviews is the improvement and deterioration rates reported for a particular approach to marital therapy labelled nonbehavioral IMT. This paper addresses the debate surrounding outcome research on IMT.

For the purposes of this paper, individual marital therapy (IMT) will be defined in contrast to conjoint marital therapy. IMT is therapy in which one or both spouses are treated separately by different therapists who do not communicate with each other about the treatment. Theoretical orientations may vary, but IMT is analogous to individual therapy with the patient's marital relationship as the focus of therapy (Gurman, 1971; Gurman & Kniskern, 1973b).

In contrast to IMT, conjoint marital therapy is typically practiced in one of four formats regardless of treatment modality:

1. Conjoint therapy where both partners are treated together by the same therapist(s).

2. Concurrent marital therapy in which spouses are treated separately by the same therapist.

3. Collaborative marital therapy in which spouses are treated separately by different therapists who communicate with one another about the treatment.

4. Conjoint group marital therapy in which both spouses are treated by the same therapist, but in the context of group therapy (Gurman & Kniskern, 1978b).

It is also important to note that it is nonbehavioral IMT which is the focus of this paper. For the sake of clarity, behavioral approaches to marital therapy are seen as distinctly different from nonbehavioral approaches. Behavioral marital therapy as described by Jacobson and Weiss (1978) and Jacobson and Margolin (1979) focuses on communication training, behavioral contracting and behavioral exchange. In simplest terms, behavioral marital therapy is everything but nonbehavioral marital therapy. Therefore, as used in this paper, nonbehavioral means a wide range of psychodynamic and/or systemic approaches as described by Gurman (1971). The psychodynamic approach is built on such concepts as transference, countertransference, insight and individual intrapsychic change while a systemic approach is concerned with interpersonal patterns, insight and action, and change in the interactional patterns of the marital dyad.

Gurman and Kniskern (1978a, 1978b) reported an improvement rate of 48% and a deterioration rate of 11.6% for nonbehavioral individual marital therapy. This outcome data, and the subsequent conclusions, evaluations and recommendations by Gurman and Kniskern have precipitated significant oppositional reappraisals and critiques. Jacobson (1978)

and Wells and Gianetti (1986) have been the primary respondents in a lively debate with Gurman and Kniskern regarding accurate efficacy rates for nonbehavioral IMT.

The debate over improvement and deterioration rates for nonbehavioral IMT has had two levels of interaction. The first is global, general and indirect and actually addresses the broader question of the treatment efficacy of all nonbehavioral marital therapy. Because nonbehavioral IMT is but a subset of nonbehavioral marital therapy, questions about IMT efficacy ultimately encompass nonbehavioral marital therapy. This level is typified by the interaction between Gurman and Kniskern (1978a, 1978b) and Jacobson (1978) in which Jacobson discounted all the outcome research of nonbehavioral marital therapy and therefore never specifically addressed the matter of IMT at all. Jacobson disregarded the outcome research cited by Gurman and Kniskern on grounds of its generally inferior research design and methodology. By devaluing all the outcome research on nonbehavioral marital therapy, Jacobson therefore implicitly questioned the outcome research on IMT.

The second level of interaction in the debate over IMT outcome research is concrete, specific and direct. This level is typified by the interaction between Gurman and Kniskern (1978a, 1978b, 1986) and Gurman, Kniskern and Pinsof (in press) and Wells and Gianetti (1986). The singular focus of this interaction is nonbehavioral IMT. Here Wells and Gianetti have reappraised each of the original studies used by Gurman and Kniskern to establish the treatment efficacy of IMT. Wells and Gianetti have concluded that the reported outcomes for IMT are based on studies with major flaws of design and implementation.

Looking at the literature on the outcome research of nonbehavioral

IMT, and the subsequent reevaluations and reinterpretations offered by the authors cited above, the reader is struck with the dissimilarity of their points of view. What is most curious about this diversity is that in fact each is addressing the same body of outcome research. The obvious question therefore is: Whose view is to be believed?

When addressing this question, most clinicians would admit to a clinical bias for or against IMT. Their bias is most probably a subjective evaluation based on clinical experience and theoretical orientation. What is needed however, is not clinical bias but empirical evidence based on objective evaluation and outcomes. Therefore, it is the position of this paper that a summary and critique of the controversy between Gurman and Kniskern, Jacobson, and Wells and Gianetti is needed and has three potential benefits: 1) a renewed attempt to determine empirical efficacy rates for nonbehavioral IMT, 2) a clarification of the requirements used to judge the adequacy of IMT outcome research, 3) a clarification of the varied goals of outcome research and its implications for determining the value of the research. Given the variety of opinions offered by Jacobson and Wells and Gianetti in their critiques of Gurman and Kniskern, it is clear that questions of efficacy can only be addressed after the controversy about research design and methodology is resolved. Likewise, questions about the worth and value of the research done to date on IMT may be determined by perceived goals of outcome research.

It is proposed that the above benefits can be realized by means of four specific tasks. The first is to examine the reported efficacy rates for nonbehavioral IMT. The critical reviews of Gurman and Kniskern (1978a, 1978b) will be the primary references used with

secondary reference to Gurman's earlier reviews (1973b, 1975). A second task is to describe the various evaluations and interpretations of this data. The evaluations of this research and data according to Gurman and Kniskern (1978a, 1978b), Jacobson (1978), and Wells and Gianetti (1986) will be described. A third task is to explain why alternative interpretations and explanations are given for the same outcome research. This can be accomplished by first noting those personal characteristics which have contributed to the diverse evaluations. In addition, this task requires the noting of the methodological and conceptual perspectives of each contributing author. Methodological and conceptual perspectives include the authors' theoretical orientation, position taken on the tension between empirical precision and clinical application, requirements for adequacy of research design and methodology, and perceived goals of outcome research. A final task, which coincides with the third, is the present author's own critique and evaluation of the debate on IMT outcome research. In this sense, I am interjecting myself in the debate between Gurman and Kniskern, Jacobson, and Wells and Gianetti. Completing the foregoing tasks will lead to summary statements about the efficacy of IMT and the value of the outcome research done to date on IMT.

Reported Outcome Data for Nonbehavioral Individual Marital Therapy

Comprehensive critical reviews of outcome research on marital therapy began with the work of Gurman (1973b). Gurman's review consisted of a detailed analysis of 26 outcome studies on marital therapy which met Gurman's minimum criteria for scientific research.

The 26 outcome studies encompassed the full range of theoretical orientations, a wide spectrum of problematic marital issues (excluding sexual dysfunction) and a variety of approaches to marital therapy, including IMT. An overall improvement rate of 66% was noted for this heterogeneous collection of modalities, approaches, patients and therapists. While Gurman judged nonbehavioral marital therapy to be moderately effective, three of the twenty-six studies reported deterioration effects in 2% of the clients. Gurman (1973b) acknowledged a positive trend toward more empirical research, but also qualified his conclusions, stating that "a more cautious and conservative estimate of the efficacy of marital therapy should be placed on these findings because no satisfactory base line of improvement for untreated marital problems has been established . . . " (p. 160). This review is significant in two respects. First, it introduced the notion of deterioration in marital therapy and second, it suggested more cautious estimates of improvement rates because of insufficient data on spontaneous remission rates. This review also highlighted the importance of a no-treatment control group in outcome research which allows an empirical determination of spontaneous remission rates.

With a desire to translate outcome research findings into clinically relevant conclusions, Gurman (1975) updated his earlier review. In 10 of 11 studies using no-treatment control groups, a variety of approaches to marital therapy (including IMT) proved superior to no-treatment. More importantly, Gurman also reviewed studies on the comparative effectiveness of several approaches to marital therapy. In nine studies cited, six major formats of marital therapy were compared: conjoint therapy, concurrent therapy, individual therapy, group therapy,

conjugal therapy, and behavior therapy. While acknowledging how each treatment format of marital therapy is not mutually exclusive, Gurman (1975) offered the following tentative conclusions:

> Conjoint therapy (nonbehavioral) has been found superior to conjoint plus individual therapy, programmed communication-enhancing methods, group therapy, concurrent therapy, and individual therapy. While the studies on the comparative effects of conjoint versus other marital therapies are still few in number, the consistent finding of the greater efficacy of conjoint methods is a striking one . . . . (p. 414)

Three years later, Gurman and Kniskern (1978a, 1978b) conducted an extensive and exhaustive review of marital therapy outcome research literature. Taken together, this and the earlier reviews have proved to be a benchmark for subsequent reviews and critiques. A significant aspect of these critical reviews was their focus on the effectiveness and effects of IMT. Based on an extensive analysis of 36 outcome studies (8 conjoint, 7 individual, 15 conjoint group and 6 concurrent), IMT was rated as least effective when compared with other marital therapy modalities. In summarizing the results of the foregoing studies, Gurman and Kniskern concluded that IMT produced improvement in 48% of its clients as compared to a 65% improvement rate for conjoint marital therapy. Furthermore, IMT had twice the rate of deterioration as compared to conjoint approaches to marital therapy (i.e., 11.6% versus 5.6%). Gurman and Kniskern also pointed out how outcome criteria were based on patient and therapist reports and described the criteria for deterioration as suicide, lower marital satisfaction, global change ratings, increased alcohol consumption, lower self-concept, delinquent recidivism and worsening presenting problems.

Results from comparative marital studies appeared to be consistent with single-group studies. Based on their analysis of the data from 25

studies, Gurman and Kniskern (1978b) noted that in 44 comparisons of conjoint therapy (conjoint marital therapy and conjoint group marital therapy) with all other marital therapy methods (including IMT), conjoint therapy emerged superior in 31 comparisons, no different in 11, and inferior in 2. In percentages, this meant that conjoint therapy was superior in 70% and inferior in only 5% of the comparisons. Gurman and Kniskern (1978b) came to the following conclusion about comparative studies:

> The low rate of positive outcomes and high rate of deterioration as a result of individual marital therapy noted earlier are not contravened by the results of the comparative studies. . . . In fact, individual treatment emerges superior to other therapies in only 10 percent of the comparisons and superior to conjoint or conjoint group therapy in only 5 percent. (p. 835)

Alternative Evaluations of the Outcome Research on Individual Marital Therapy

The Gurman and Kniskern (1978a, 1978b) reviews continue to be cited as the primary references for outcome research on nonbehavioral marital therapy and nonbehavioral IMT, but interpretations of the data on which they based their conclusions have varied considerably. Evaluations of this data by Gurman and Kniskern (1978a, 1978b, 1986), by Jacobson (1978), and by Wells and Gianetti (1986) will now be described.

Gurman and Kniskern

Gurman and Kniskern's evaluation of the outcome research on IMT is best presented in chronological order. The use of a chronological sequence is important because it demonstrates how their position has changed over time. The Gurman and Kniskern (1978a, 1978b) initial evaluation will be noted first followed by the most recent evaluations (Gurman & Kniskern, 1986; Gurman, Kniskern & Pinsof, in press).

After pointing out the lower improvement rate and the higher deterioration rate of IMT, Gurman and Kniskern (1978b) concluded: "In conjunction with the unimpressive improvement rate noted earlier for individual marital therapy, there appears to be little empirical basis for its continued practice" (p. 832) Further, they made this evaluative statement in response to the efficacy data:

> We believe these data are persuasive enough to question routinely the value of individual therapy for marital problems. The burden of empirically demonstrating the utility or advantages of such treatment rests now on its advocates. (p. 835)

In response to criticism of their conclusions by Wells and Gianetti (1986), Gurman and Kniskern (1986) and Gurman, Kniskern and Pinsof (in press) have revised the evaluations and interpretations of the earlier review (Gurman and Kniskern, 1978b). In regard to the efficacy of IMT, Gurman and Kniskern (1986) have reanalyzed the data and have offered the following reinterpretation:

> There is suggestive, and consistent, evidence that marital therapy which combines a predominant use of IMT with other (usually conjoint) treatment formats may be less effective than marital therapy which combines a secondary use of IMT with other (usually conjoint) treatment formats. (p. 58)

Regarding the empirical status of IMT and conjoint marital therapy, Gurman and Kniskern (1986) have come to the following conclusions:

> 1. There is very little acceptable empirical evidence of the inefficacy of IMT. 2. There is no evidence, acceptable or otherwise, of the efficacy of IMT. 3. There is a large body of acceptable evidence of the efficacy of conjoint marital therapy. The null hypothesis regarding IMT (i.e., no difference between IMT and no therapy) has not yet been disconfirmed, whereas the null hypothesis regarding conjoint marital therapy has been disconfirmed repeatedly. (p. 59)

They also concluded:

> Since IMT is currently . . . an (essentially) untested method, and since conjoint marital therapy is a well tested method with demonstrably positive effects, there is ample warrant for the position that conjoint marital therapy can, for now, be considered

the treatment of choice for marital problems. (p. 59)

Gurman, Kniskern and Pinsof (in press) have qualified the above conclusions when they concur with the note by Todd and Stanton (1983) that "it is important not to generalize. . . . it may be premature to claim superiority for marital treatment when the presenting problem is an 'individual' one, e.g., wife's depression" (p. 94).

Jacobson

Jacobson's first response (1978) to the Gurman and Kniskern (1978a, 1978b) review was manifestly critical. In his introduction, Jacobson (1978) stated, "marital therapy, like individual psychotherapy, has developed largely in an empirical vacuum. The analogy between the two modalities is striking, although perhaps not surprising" (p. 395). Jacobson (1978) became more specific and concrete when he stated his reaction to the uncontrolled outcome studies used by Gurman and Kniskern to establish the efficacy of nonbehavioral individual marital therapy: "From a scientific standpoint such reports are of limited utility, since they neither objectively demonstrate the efficacy of their procedures nor further our understanding of the mechanisms by which such positive changes might come about" (p. 396).

It is important to remember here that Jacobson's evaluation was directed at nonbehavioral marital therapy in general. It appears logical, however to apply these evaluations to nonbehavioral IMT, a subset of nonbehavioral marital therapy.

Jacobson (1978), while admitting to a behavioral theoretical bias, asserted that his strongest bias "is a belief in the importance of an experimental analysis of marital therapy procedures" (p. 397). Based on empirical, scientific principles of research, Jacobson then essentially

discounted the review and conclusions of Gurman and Kniskern (1978b), which included nonbehavioral IMT. As stated by Jacobson (1978), "neither perspective [referring to psychoanalytic and systems theory] can claim even a single outcome experiment investigating its effectiveness" (p. 397). Jacobson stated further that uncontrolled outcome studies may "generate hypotheses which can later be tested in controlled investigations" (p. 403), but his major emphasis was that the majority of research on the effectiveness of nonbehavioral marital therapy though characterized by clinical vigor has been lacking in empirical rigor (Olson & Sprenkle, 1976).

While Jacobson does not offer a specific current evaluation of the outcome research on nonbehavioral IMT, his most recent publications seem to indicate degrees of equivocation and moderation from a previous, more rigid empirical point of view (1985a, 1985b). While in earlier writing (Jacobson, 1978) he seemed to have absolutely disregarded research which was not "scientific and empirical," these more recent writings (1985a, 1985b) indicate a hint of receptivity to the utility of research which does not necessarily meet textbook criteria (i.e., random assignment, control group) for design and methodology. Recently, Jacobson (1985a) has also suggested that outcome studies be "reported in ways that are clinically meaningful" (p. 150). Jacobson's (1985a) emphasis on "clinical meaningfulness" also included a necessary compromise in research design:

> There are two aspects of research design about which there can be no compromise: the use of random assignment, and inclusion of a control group. However, there are experimental questions which can be answered without either; these types of questions are typically reserved for the later stages of research. . . . There is a stage in an outcome research program where random assignment not only can, but must, be dispensed with. To put it another way, the treatment package must be made more flexible so that its

> generalizability to conditions which mirror clinical practice can be evaluated. (p. 153-154)

Jacobson's slight turning from empirical orthodoxy is evident though he continues to maintain that empirical rigor basically precedes clinical usefulness. Internal validity is still paramount for Jacobson, so the aspects of random assignment and a control group are only excluded in later stages of research.

Wells and Gianetti

The recent evaluation of the outcome research on IMT by Wells and Gianetti (1986) contains their part of a lively and heated debate with Gurman and Kniskern. While acknowledging that the ineffectiveness of IMT has become like "an article of faith in family therapy circles" (Wells & Gianetti, 1986, p. 43), they have soundly rebuked Gurman and Kniskern for promulgating this doctrine. In reappraising the research studies used by Gurman and Kniskern to establish the ineffectiveness of IMT, Wells and Gianetti have disregarded almost all of these studies on the basis of "gross flaws in design and implementation" (p. 43). Wells and Gianetti (1986) have taken a dim view of the marginal research purporting the ineffectiveness of IMT and have argued rather that because of inadequate evidence, "no conclusions can be reached concerning either the absolute or relative effectiveness of IMT" (p. 43). Even more concise and concrete in their criticism, they have offered the following evaluative statements:

> As our reappraisal will make clear, we believe that Gurman and Kniskern's conclusions are unfounded and that their review of the research literature in this area [IMT] is permeated with errors of fact, critical omissions and selective reporting. We will not contend that the available evidence <u>supports</u> the effectiveness of IMT but, rather, that any case against it is completely <u>unproven</u>. (Wells & Gianetti, 1986, p. 44)

Explaining Alternative Evaluations According to Personal Characteristics

Given the diversity of response to the critical reviews by Gurman and Kniskern (1978a, 1978b) it is difficult to arrive at a conclusive decision about the efficacy of nonbehavioral IMT. All that can be concluded logically is that the diversity reflects personal characteristics and a variety of methodological and conceptual perspectives applied to the same body of research.

Polemical debates appear to attract people with unique personality traits and characteristics. It may be argued that these personality traits and characteristics help explain in part the diversity of evaluations given for the outcome research on IMT. One such personality characteristic is that of contentiousness.

By their own admission, the contentious characteristics of the authors under discussion fosters lively and interesting debate. Previous charges and countercharges between Wells and Dezen (1978a, 1978b) and Gurman and Kniskern (1978d), or between Jacobson and Weiss (1978) and Gurman and Knudson (1978), Gurman and Kniskern (1978c) and Gurman et al. (1978) are vivid examples. Kniskern himself (1985) conjectured that much of the disagreement between Jacobson and Gurman and Kniskern has been simply a matter of contentiousness. In a direct reply to Kniskern (1985), Jacobson (1985b) appeared to agree, but then also suggested other explanations:

> Speaking for myself, there is no doubt that I am mellower than I was seven years ago; moreover, the trait attribution which relegates the acrimony in the previous debate to the contentiousness of Gurman, Knudson, and Weiss is quite appealing. However, in all honesty, I suspect that the correct attribution is a combination of the "different subject matter," as mentioned by Kniskern, and the shift in contingencies that has occurred in the field. (p. 163)

When considering conceptual and interpretive differences, however, it is preferable that clashes of personality be laid aside. Gurman and Kniskern (1986) seem to concur when they have stated recently:

> There are many valuable lessons for family therapy researchers [referring here to the debate over Eysenck's 1952 work], . . .not the least of which is the importance of not allowing conceptual and interpretive differences to be overrun by personal acrimony or vendettas. (p. 52)

It is the position of this paper that an element of contentiousness has played a significant role in the evaluative and interpretive conclusions of the debaters. Smith, Glass and Miller (1980), seemed also to share this point of view when they made this general comment about the nature of psychotherapy outcome research: "Polemics and rhetoric--saying it loud, often and categorically—more frequently characterize the literature than does the careful, dispassionate and fair evaluation of the evidence" (p. 3). The roots of contentiousness in the IMT debate may be found in the combination of a gadfly's involvement and the general forensic skill of the participants.

The Gadfly Characteristic

The diverse outlooks on the outcome research for nonbehavioral IMT may have been sharpened by the activity of Gurman as a sort of "entrepreneur gadfly." The gadfly habitually seizes upon the moment to act as a provocative and hopefully constructive stimulus to direct criticism at the status quo. Such provocation assures a response which begins a point and counter-point response cycle. While such gadfly activity may have originated in genuine criticisms and differences, it often carries a sting which is apt to be energized by unfortunate misrepresentation and misunderstanding, overheated rhetoric, forensic maneuvering and outright contentiousness. Jacobson and Wells and

Gianetti are provoked by the gadfly Gurman. With Gurman as provocateur and gadfly, Wells and Gianetti and Jacobson become the offended responders.

Gurman's doctoral dissertation eventually evolved into his first major review (1973b) and he (together with Kniskern) has distinguished himself by attempting to clarify and synthesize research done by others (Gurman, 1975; Gurman & Kniskern, 1978a, 1978b, 1981, 1986,) while himself adroitly sidestepping personal investment in the actual research enterprise.

In their most recent rejoinder to the criticisms of Wells and Gianetti (1986), Gurman and Kniskern (1986) have drawn an analogy between themselves and Eysenck. By quoting Bergin and Lambert's (1978) historical critical re-assessment of Eysenck (1952), Gurman and Kniskern have cast Wells and Gianetti in the role of Bergin and Lambert with themselves cast as Eysenck. Just as gadfly Eysenck (1952) questioned the improvement rates of individual psychotherapy and noted the reality of deterioration in some patients, Gurman and Kniskern have done the same with IMT. In analogous fashion, Wells and Gianetti have criticized these conclusions after a reanalysis of the outcome data, just as Bergin and Lambert (1978) accused Eysenck of mishandling and misinterpreting the outcome data for individual psychotherapy. Gurman and Kniskern (1986) have also noted how the debate aroused by Eysenck seemed endless because of the provocative nature of the results and subsequent claims and counterclaims of each. Considering the analogy, Gurman seems to fit and has nourished and relished a gadfly's role.

Forensic Skill

The most recent phase of the debate between Wells and Gianetti (1986) and Gurman and Kniskern (1986) has confirmed Gurman's skill in forensic maneuvering. The debate had appeared to end with Gurman and Kniskern renouncing their earlier conclusions about IMT and agreeing with the Wells and Gianetti conclusions, only to be reawakened when Gurman and Kniskern qualified their agreement. Gurman and Kniskern now maintained that however crude or contaminated the IMT outcome studies were, they have been nevertheless "heuristically useful" (Gurman & Kniskern, 1986, p. 56). Therefore, Gurman and Kniskern have proclaimed, "we would, however, not be inclined, as are our critics, to throw out the proverbial baby (with the contaminated bath water) so quickly" (p. 57). Gurman is clearly a master of forensic skill with Jacobson and Wells and Gianetti more than willing to summon their own not inferior forensic abilities. Apparently none of the participants is yet ready to concede defeat at this juncture.

Suggesting that the elements of the gadfly or provocateur characteristic and forensic skill have influenced the debate over the efficacy rates of nonbehavioral IMT does not detract at all from the value and usefulness of the participants' critical reviews and reappraisals. In fact, it could be argued that our present understanding about efficacy rates for IMT has been well served by these personal characteristics. As Gurman and Kniskern (1986) have noted, "multiple views ('polyocular' is the term in current vogue) of a complex body of research literature are probably inevitable since, thank the gods, reviewers are not interchangeable clones" (p. 58).

Explaining and Critiquing the Diverse Evaluations According to Methodological and Conceptual Themes

Some consistent themes emerge when attempting to critically evaluate the diverse explanations and interpretations offered for the outcome research on nonbehavioral IMT. These themes capture the range of empirical and clinical points of view each reviewer brings to bear on the same body of research and data. It can be argued that the range of perspectives held by the debaters provides an explanation for the dissimilarity of interpretations given for IMT outcome research. These perspectives appear in each to be the compilation of their theoretical orientation, the tenuous balance of their view of scientific precision and clinical application, their perceptions of minimal standards for outcome research and their understanding of the goals of outcome research. Admittedly these themes have an impact on all outcome research but particularly impact the evaluation of IMT.

Theoretical Orientation

The reviewers' individual theoretical orientation has had some degree of influence on the manner in which outcome research is evaluated and interpreted. While a reviewer's theoretical perspective does not have the power to change the actual data, theoretical outlook may have the potential to influence degrees of acceptance or conceptual orientation to outcome research.

As stated earlier, the present author's clinical/theoretical inclination is to accept a conclusion of lower efficacy rates and higher deterioration rates for nonbehavioral IMT. This theoretical position views individual functioning psychodynamically, but sees the interaction of the marital dyad systemically. If the presenting complaint is

marital, then a systemic approach is indicated in which patterns and styles of the marital dyad are the focus of intervention. When appropriate, the intrapsychic dynamics of spouses may be examined. To conduct marital therapy with only one spouse present runs the risk of collusion with or against the absent spouse. Conducting individual therapy (whatever the orientation) with one spouse, in which the therapist allows the spouse to focus on marital difficulties, is to support a subtle form of "acting out" (A. Panajian, personal communication, October 25, 1985; B. Narramore, personal communication, November 13, 1985). For those marital problems which are impervious to systemic interventions because of the inadequate psychological development of spouses, then individual psychodynamically oriented therapy for each spouse would be indicated as a transitional step before returning to systemic marital therapy.

The theoretical biases of Gurman and Kniskern, Jacobson and Wells and Gianetti need to be taken into consideration in any appraisal of their critique and interpretation of each other's work. It is futile to argue for the right or wrong of theoretical positions, but it is important to take theoretical positions into account when noting the dissimilarity in authors' interpretations of the same body of outcome research.

Jacobson and Wells and Gianetti. Jacobson's, and Wells and Gianetti's theoretical orientation is behavioral (Jacobson, 1978; Jacobson & Weiss, 1978; Jacobson & Margolin, 1979; Wells & Dezen, 1978a; Wells & Dezen, 1978b). Consistent with this theoretical orientation, more significance is placed on empirical questions than clinical concerns when outcome research is conducted. With a behavioral

orientation, concepts of behavioral exchange, behavioral contracting, communication and problem-solving training are primary. The underlying foundation of these concepts is a committment to empiricism and heavy dependence upon experimental methods as a means of acquiring a greater understanding of behavior. Experimental methods include objective observation, scientific controls, precision in measurement and statistical analyses with a goal of easy replication. When these methods are applied to the outcome research on IMT, one could predict more stringent standards for acceptance of adequate outcome research. Undoubtedly, some of the outcome studies on nonbehavioral IMT would be suspect based on stringent standards for outcome research.

Gurman and Kniskern. The theoretical orientation of Gurman is more difficult to decipher primarily because he rarely states his position explicitly. Indirect or implicit references are made in some of his publications (Gurman, 1971; Gurman & Kniskern, 1978a, 1978b, 1981). He was most explicit (Gurman, 1981) when he stated that psychodynamic thinking, originating in object relations theory, is "the most appropriate theoretical foundation for an integrative understanding of marital interaction, dynamics, and change" (p. 453). But in the same publication, he also acknowledged that psychodynamic theory does not address the necessary technical aspects of marital therapy, and therefore he advocated an interpersonal-psychodynamic framework. It appears that this integrative framework combines psychodynamic personality functioning (intrapersonal) with systemic functioning (interpersonal). He clearly sees a dichotomy between individual and relationship change as artificial and therefore advocates an interpersonal-psychodynamic framework which provides the opportunity for

pragmatic and eclectic therapeutic interventions. By advocating what is in fact a pragmatic eclecticism, Gurman and Kniskern (1981) inferred that the interests of clients should become the focus of attention in contrast to theoretical supremacy. To decipher Gurman's theoretical point of view is to understand how he would be expected to have some inclination to emphasize clinical concerns as much or more than empirical procedures in outcome research. Therefore, Gurman's minimum standards for outcome research may be perceived as being less stringent than they would be for the behaviorists.

Internal versus External Validity

Producers, evaluators and consumers of research are frequently confronted with the tension between internal and external validity. Internal validity is defined as the extent to which an experiment rules out alternative explanations for outcomes. External validity is the extent to which the outcomes of an experiment can be generalized. Kazdin (1980) recognized this tension and noted that there is often an inverse relationship between experimental precision and generalization. The relative importance of internal validity in relation to external validity is a critical factor in evaluating outcome research.

Gurman and Kniskern. Of crucial importance to an understanding of Gurman and Kniskern's perspective on outcome research is their view that the empirical issues (internal validity) and clinical issues (external validity) of research are "synergistic" and not "antagonistic" (Gurman & Kniskern, 1981, p. 744). Their perceived goal of research is that empirical questions should be addressed from a clinician's perspective and clinical issues from an empirical perspective. While noting the temptation to view empirical and clinical issues as mutually

antagonistic, they have stated their viewpoint as follows:

> If one takes the position as we do, that clinical concerns and empirical concerns are not antagonistic, but are, in fact, synergistic and should be indistinguishable, then the apparent dilemma is immediately resolved. (Gurman & Kniskern, 1981, p. 744)

Unfortunately, Gurman and Kniskern do not provide a definition of synergism. However, the term is usually understood to mean the interaction of two factors which achieve an effect of which each is individually incapable. It would appear, then, that Gurman and Kniskern (1981) have attributed equal importance and significance to both internal validity and external validity. This is an unfortunate comparison, since internal validity is viewed sequentially as of greater importance than external validity (Kazdin, 1980). Questions of applicability and generalization can only be logically entertained if the relationship of independent and dependent variables is without ambiguity. Once a clear causal relationship between independent and dependent variables has been established, an accumulation of studies which encompass external validity factors can be conducted.

Jacobson. In contrast to Gurman and Kniskern (1978b), Jacobson (1978) has advocated a position which places an ultimate priority on empirical issues (internal validity), to the degree that issues of clinical relevance and applicability (external validity) cannot be entertained until empirical issues have been resolved. He seems to have perceived empirical and clinical issues as antagonistic and adversarial, i.e., that clinical issues and concerns are at odds with the empirical goals of outcome research. Jacobson (1978) clearly stated that internal validity (eliminating alternative explanations for change through appropriate research design) assumes a sequential priority over external validity (the ability to generalize the results to other clients,

settings, et cetera). Jacobson's point of view seems to represent sound principles of research design, yet his position also raises the question of the minimum criteria required to establish internal validity.

Wells and Gianetti. Wells and Gianetti (1986) have taken a position similar to that of Jacobson. Their primary critique of Gurman and Kniskern (1978) is that of inadequacy in design and methodology. Clearly, Wells and Gianetti (1986) adhere to the priority of internal validity. Minimum criteria for outcome research will now be discussed as they relate to divergent views of the efficacy rates for IMT.

Minimum Criteria For Individual Marital Therapy Outcome Research

Standard texts in research design and methodology (Kazdin, 1980; Keppel, 1982) clearly describe basic textbook criteria for sound outcome research. These criteria include a no-treatment control group, random selection of subjects, random assignment to treatment conditions, uncontaminated independent variable, clearly defined dependent variable, multi-dimensional outcome assessement, follow-up, and appropriate statistical analyses. Whereas there are generally accepted minimum standards for outcome research, the different interpretations and evaluations of the outcome research on IMT appear to be a function of diversified application and committment to these minimum standards. Based on the standard minimum criteria for all psychotherapy outcome research generally it is worthwhile to clarify by examination and critique how these criteria were specifically applied to judge the adequacy of IMT outcome research. Those criteria that apply will be discussed.

Control Group. The use of a randomly assigned no-treatment control group is usually seen as paramount in any effort to establish an

unambiguous causal relationship between independent and dependent variables. By using a control group, conclusive statements can be offered about the efficacy of treatment. However, participants in the debate over the efficacy of IMT place varying degrees of importance and value on the use of a no-treatment control group. According to Jacobson (1978), the inclusion of a no-treatment control group is a minimum requirement in all outcome research and he therefore totally discounted the conclusions arrived at by Gurman and Kniskern (1978a, 1978b). He stated his view as follows:

> One hundred uncontrolled investigations of a given treatment, all suggesting improvement among treatment couples, constitute 100 uninterpretable studies; even when considered together, the cumulative total of these studies fails to establish causal relationships between a given treatment and positive changes. A related point concerns comparative studies which compare two types of marital therapy; such studies do not preclude the necessity for waiting-list control groups. (Jacobson, 1978, p. 401)

In a more recent publication, Jacobson (1985a) has again asserted that there can be no compromise about the inclusion of a control group but he has also suggested that an alternative form of control group could be a standard, established treatment program, e.g., behavioral marital therapy. This suggestion appears to be a welcome compromise, but some may question whether behavioral marital therapy is itself sufficiently standardized, established and efficacious (Gurman & Kniskern, 1978c) to serve such a role.

Wells and Dezen (1978a, 1978b) and Wells and Gianetti (1986) have taken note of the importance of control groups, but are less adamant than Jacobson. They take this position from an apparent assumption that spontaneous remission rates for marital dyads are the same or worse than for individuals. In so doing they do not criticize Gurman and Kniskern on this criterion.

Gurman and Kniskern (1978a, 1978b) acknowledged that the gross improvement ratings reported for nonbehavioral IMT (48% and 11.6% deterioration rates, respectively) are from studies without control groups. Without a no-treatment control group, it is impossible to state conclusively that treatment caused improvement or deterioration, though it can be stated that such changes occurred during the course of treatment. Gurman and Kniskern (1978b, 1981) appeared to be satisfied with this limitation since they have noted that results of comparative studies are noticeably consistent with gross improvement ratings.

While the inclusion of a no-treatment control group may be ideal, there are ethical, procedural and conceptual difficulties to be taken into consideration (O'Leary & Borkevec, 1978; Wilkins, 1983, 1984; O'Leary & Turkewitz, 1978). Withholding treatment from distressed couples is ethically indefensible, the matching of treated patients and control group patients is at best haphazard and the potential for a truly uncontaminated control group is questionable. Disputing the conclusions of Gurman and Kniskern (1978a, 1987b) because of their reliance on studies without control groups is understandable, but totally eliminating or discounting their conclusions on this singular criterion seems overdone.

Uncontaminated Independent Variable. A clear, concise and customary application of treatment, which allows for replication, is another important criterion in evaluating outcome research. Jacobson (1978) and Wells and Gianetti (1986) have used this criterion to question the outcomes reported by Gurman and Kniskern (1978a, 1978b). Jacobson (1978) has preferred treatment interventions which are clearly spelled out, programmed and instituted with the goal of easy

replication. Gurman and Kniskern have understood that strictly programmed intervention procedures or the following of published treatment manuals may bring outcome research closer to acceptance as "hard science" but they have also viewed this as a compromise of clinical reality. Gurman and Kniskern hold to a necessary interaction between clinical and empirical issues in order to insure that scientific research stays in realistic contact with clinical practice.

While Jacobson's criticisms are global and apply to nonbehavioral marital therapies in general, Wells and Gianetti's (1986) criticisms apply specifically to IMT. In their detailed analysis of the seven uncontrolled outcome studies on IMT, Wells and Gianetti argued convincingly that in 5 of the studies, treatment was significantly contaminated (IMT in conjunction with conjoint therapy) or uncustomarily brief and therefore unrepresentative of IMT (three sessions or less). Similarily, Wells and Gianetti questioned 7 of the reported comparative studies on the basis of treatment contamination. Gurman and Kniskern (1986) have since responded by stating treatment was often very brief because of attrition or drop-outs and also contended that Wells and Gianetti (1986) have exaggerated the contamination. On both variables, drop-outs and contamination, Gurman and Kniskern have also argued for the realism of these factors in clinical practice. Ultimately however, they have agreed with the conclusions of Wells and Gianetti (1986) who have stated: "No conclusions can be reached concerning either the absolute or relative effectiveness of IMT" (p. 43).

On the basis of the criterion of an uncontaminated independent variable, the conclusion of Wells and Gianetti (1986) appears to be accurate. Clearly the studies used to arrive at efficacy rates for IMT

show evidence of treatment contamination and brevity. In this regard, the studies cited for outcomes of IMT do not seem to genuinely represent an IMT approach to marital problems.

Statistical Procedures. The Gurman and Kniskern (1978b) pattern of "box score" analysis of comparative studies is a procedure which has been questioned. When Gurman and Kniskern (1978a, 1978b) conducted their critical review of the outcome research on marital therapy, they devised a rating scale in order to establish minimum criteria for inclusion. Each study was assessed according to a 14 criteria Design Quality rating schedule. Each criterion had a points rating allowing studies to be scored as poor, fair, good or very good in design quality (Gurman & Kniskern, 1978b).

Box score analysis refers to the practice of mixing good and poor quality studies together but nevertheless summarizing the results. Gurman and Kniskern (1978b) admitted that this practice may be methodologically unfashionable or unpopular, but also defended the procedure. First, they argued that the results of poor quality studies are consistent with better quality studies. Second, they pointed out that gross improvement rates and results of controlled marital studies (superior in design quality) are in line with comparative results. Third, they insisted that consistent results on gross outcome studies, comparative studies and controlled outcome studies seem to indicate a clinical phenomenon which supersedes issues of design quality. Fourth, they also defended the process of box score analysis from a perspective that sees the arbitrary elimination of studies weak in a particular design feature as wasteful. Gurman and Kniskern (1978b) have preferred to interpret obscure studies in the context of other specific clear

results while still admitting the existence of studies based on both good and poor research design. Fifth, they have concluded that alternative, precise analytic techniques, such as meta-analysis (Smith & Glass, 1977) have verified the box-score results about the efficacy of nonbehavioral marital therapy (Gurman, Kniskern & Pinsof, in press).

Wells and Dezen (1978a) and Wells and Gianetti (1986) have strongly disagreed with the practice of box score analysis. They view it as self-deceptive to believe that a series of logically flawed studies can converge toward any useful or meaningful conclusions. Wells and Gianetti (1986) were accurate in noting that 7 of the 8 comparative studies on IMT are of poor design quality, with the remaining one rated as good.

Gurman and Kniskern's comparative logic appears faulty if box score analysis is used to make definitive conclusions about the efficacy of IMT. A conglomeration of studies poor in quality of design does not provide a sound basis for conclusions. Box score analysis of poorly designed studies can be profitable, however, if used as a spur to further investigation and research.

Multi-Dimensional Outcome Assessment and Follow-up. Another significant criticism of the studies used by Gurman and Kniskern (1978a, 1978b) to establish the efficacy rates of IMT has been the predominance of self-reports used in outcome assessment. Wells and Gianetti (1986) have suggested that outcome assessments based primarily on client evaluations lack objectivity and empirical precision. They have urged that some form of independent assessors must be used in conjunction with both client and therapist reports. Jacobson (1978, 1985a) has taken a similar view.

Not only have Gurman and Kniskern (1981) consistently advocated the use of multidimensional criteria (empirical emphases) for outcome assessment, they have also advocated the use of multiple perspectives (clinical emphases) for outcome assessment. While they have accepted the empirical value of multidimensional criteria, they have also advocated the appropriate use of clinical perspectives (self-reports or therapist reports) in assessing outcomes. Gurman and Kniskern see the issue of whether "objective is more accurate than subjective" as artificial. Since they have questioned whether any outcome criterion is truly objective, they prefer therefore to speak in terms of "realness." In their more recent review (Gurman & Kniskern, 1986) they have taken the position that objective changes are no more real than are patient reports.

Whether objective or subjective assessment of outcomes is most accurate is analogous to the question of whether facts are different from truth. A debate on this dimension poses the question of whether internal subjective experiences of truth can be measured or quantified through objective measurement. It is the position of this paper that changes brought by marital therapy should be expected to have both objective and subjective qualities. Observable, measureable changes in external behavior are likely, but internal changes which are highly subjective are also probable. The apparent absence of one level of change, (i.e., external behavior which is measureable) does not necessarily preclude internal change which is experienced subjectively.

Debate over objective versus subjective outcome assessment also raises another question. What is the locus of change for marital therapy: Is it interpersonal or intrapsychic? Jacobson (1978) and

Wells and Gianetti (1986) seem to exclude intrapsychic change, and focus on the measurement of interpersonal changes whereas Gurman and Kniskern (1978b, 1981, 1986) seem to acknowledge the reality of both intrapsychic and interpersonal dimensions. They are at least consistent with their theoretical orientations. Unfortunately, the IMT outcome research to date reveals an inadequacy in multi-dimensional outcome assessment. Instruments for assessing change in external interpersonal behavior generally lack validity, and measures for intrapsychic change typically rely on the questionable practice of self-reports.

Follow-up assures the genuine impact of treatment over time. However the validity of these results often rests on response rates and on whether follow-up was done retrospectively. In this regard, Wells and Gianetti (1986) have noted how 5 of 7 uncontrolled outcome studies and 7 of 9 comparative studies used by Gurman and Kniskern (1978a, 1978b) conducted follow-up long after the end of therapy (seven years in one study) or as an afterthought. This introduces a host of potential problems for questions of internal validity. In addition, low response rates were noted by Wells and Gianetti (1986) in 3 of the uncontrolled outcome studies.

Once again, the criticism of Wells and Gianetti is not easily refuted. The studies employed by Gurman and Kniskern (1978a, 1978b) to establish efficacy rates for IMT relied heavily on retrospective reporting. Retrospective reporting introduces a number of additional threats (i.e., history, maturation) to internal validity. The lack of objective outcome assessment and the strong reliance on retrospective reporting fosters equivocal conclusions about the effectiveness of IMT.

Based on his definition of minimum requirements for outcome

research, Jacobson (1978) concluded: "According to the standards for outcome research . . . these reports [i.e., of nonbehavioral marital therapy] do not qualify as scientific investigations" (p. 403). Likewise, Wells and Gianetti (1986) have criticized the original conclusions of the Gurman and Kniskern (1978a, 1978b) reviews as having been made on the basis of what they perceived as flawed, inadequate and inconclusive studies. As noted earlier, Gurman and Kniskern (1986) and Gurman et. al (in press) have responded to this criticism and changed their earlier position. With these most recent appraisals of the outcome research, Gurman and Kniskern have concluded there is little acceptable empirical evidence either for or against the efficacy of IMT.

Conclusion Regarding Efficacy Rates For Nonbehavioral IMT. It is apparent that on the basis of inadequate research design and methodology, accurate efficacy rates for nonbehavioral IMT cannot be clearly established. To date, research on IMT simply does not satisfy minimum empirical criteria for outcome research. To date, Wells and Gianetti, Jacobson, and Gurman and Kniskern all appear to agree with this conclusion. However, one final aspect of their debate regards the value, merit or significance of the outcome research on IMT to date. It is the position of the present author that there is clinical significance in the IMT outcome research if the goals of research include more than empirical verification.

Goals of Research

Another basic conceptual assumption underlying outcome research is the perceived goal of research. For example, is the goal verification and definitive conclusions to a question or is it exploration and continued investigation of a concern? The goals of research will first

be described as they are conceived by Gurman and Kniskern, by Jacobson, and by Wells and Gianetti.

The perceived goals of research for nonbehavioral IMT outcome research are particularly salient considering the generally inadequate research design and methodology of IMT studies to date. The questionable value of flawed and inadequate outcome research (i.e., nonbehavioral IMT) can be assessed by understanding goals of outcome research. The present author takes the position that a comprehensive understanding of the goals of research ultimately affirms the value of the Gurman and Kniskern (1978a, 1978b) critical reviews of outcome research for IMT, regardless of whatever shortcomings may exist in the design quality of the studies used.

Gurman and Kniskern. The synergistic perspective of Gurman and Kniskern (1978b) allows for a significant degree of clinical latitude in determining the value of outcome research. While well aware that their most persistent critics (Jacobson, 1978; Wells & Dezen, 1978a; Wells & Gianetti, 1986) question the methodological adequacy of the studies used in their assessment of the efficacy of marital therapy, Gurman and Kniskern (1981) stated:

> The issue does not involve disagreement about the methodological adequacy of specific studies, but rather addresses the matter of whether such studies, which we agree are deficient, can be instructive nonetheless. . . . Our response to the critiques offered by our colleagues is, simply, that they are correct on methodological grounds, but that they have used the wrong criterion for judging the usefulness of such research findings. We certainly do not endorse the view that family therapy research should treat methodological issues casually . . . . Nevertheless, we believe that, like love, methodological adequacy is not enough. If research on family therapy is to have clinical impact, then inferences about the efficacy of our treatments must also be characterized by logical adequacy. (p. 751-752)

Unfortunately, Gurman and Kniskern do not give a clear definition

here of what they mean by logical adequacy but perhaps their meaning can be inferred. They appear to mean the consistent pattern of results between poorly designed studies with those of better quality and between uncontrolled and comparative outcome studies. As has been noted earlier, there is no strength in numbers alone if a conglomeration of poorly designed studies is used to draw conclusions. Conclusions from a large number of poorly designed studies cannot be logically collapsed into one sound conclusion.

Jacobson. Key concepts in the original writings of Jacobson (1978) are rigorous methodology, scientific inquiry, and experimental analysis. Jacobson is orthodox and evangelistic in his zeal for rigorous goals for outcome research. His goal is to improve the quality of marital therapy from the level of preliminary study to that of systematic experiment. Unfortunately, Jacobson seems to be occupied with the preferred ideal while altogether ignoring present reality.

Unfortunately, nonbehavioral marital therapy research is still at the level of preliminary study and is simply not advanced enough at this point to invest all of its energies in systematic experimentation. Instead, energies need to be invested in discovering and generating new hypotheses which will eventually lead to more sophisticated empirical study and ultimately to verification. The tight controls required of systematic experimentation would seem to squelch the spirit of problem formulation and exploration.

Wells and Gianetti. Wells and Gianetti (1986), and before that Wells and Dezen (1978a, 1978b) share Jacobson's goal for research. Scientific controlled experiments are the intent with the goal of easy replication. This intention is understandable in terms of conventional

criteria for research design, yet these criteria may not be sufficient for determining usefulness. Gurman and Kniskern (1978d) have accused Wells and Dezen of "technolatry" and "methodolatry". Technolatry refers to the technology of treatment at the expense of interpersonal or interactional factors. Methodolatry is the tendency to worship traditional methodology to the extent that scientific curiosity supplants any clinical concern. Gurman and Kniskern have not only held that an emphasis on the technology of treatment tells us very little about the efficacy of marital and family therapy as it is really practiced, they have also opposed the view that scientific procedures must supplant clinical concerns.

Appraising the Appraisals: The Value of the Individual Marital Therapy Outcome Research

The questionable value of methodologically incomplete and flawed outcome research on IMT is further complicated by the general disagreement amongst psychologists about the goals of psychotherapy research in general. As Kuhn (1970) noted, psychology as a discipline lacks a common paradigm—a shared set of fundamental beliefs, exemplars, and symbolic generalizations. Therefore, disagreements about what defines good research are inevitable. Kazdin (1986) has agreed with Kuhn and noted the tension that exists between statistical and clinical significance. Conclusions about the relative effectiveness of psychotherapies are based on statistical evaluations; but clinical significance or usefulness is of an entirely different genre. A major problem is found in the fact that there is no uniformly accepted standard procedure or measurement strategy by which clinical usefulness

can be assessed. Since statistical significance is so much easier to determine than clinical significance, researchers have tended to study what they know how to study or can study with relative ease rather than to study what is of importance to the conduct of therapy.

The present author suggests that the value of the work of Gurman and Kniskern (1978a, 1978b) can be assessed if the goal of research is perceived and understood according to certain necessary distinctions, a discussion of which follows.

Research Idealism versus Clinical Realism

It would be preferable to have studies which satisfy textbook criteria for scientific research and yet are applicable to realistic clinical settings and treatment. Unfortunately such preference is idealistic and utopian. Psychotherapy outcome research is always less precise because of its attempts to be realistic, or unrealistic because of its attempts to be scientifically precise. Unquestionably, the studies reviewed by Gurman and Kniskern (1978a, 1978b) are plagued with major weaknesses in research design and methodology, yet the studies do represent clinical realism. For example, while it was argued that the outcome studies on IMT are flawed by contamination and brevity of treatment, thus eliminating any chance for causal conclusions, the practicing clinician would probably attest to the realism of treatment contamination and brevity. Statistical significance alone is an inadequate criterion on which to judge the utility or application of IMT research.

Inside versus Outside the Profession

Smith, Glass and Miller (1980) correctly asserted that "only controlled experimental method has the power to persuade a wide audience

of contemporary scholars and educated laymen" (p. 24). The spirit of the times is clearly "prove it, document it and replicate it or we won't pay for it." As a realistic profession, psychology must meet this challenge. However, within the profession there must be the freedom to explore and generate new treatment approaches without the tyranny of proving, documenting and replicating. Our ultimate goal needs to be the presentation of controlled experimental studies which will convince consumers, third-party payers, and national health policy makers. Work such as that of Gurman and Kniskern, of Jacobson, and of Wells and Gianetti represents an important intermediate step. There is an appropriate time and place for empirical rigor as well as clinical vigor.

Publication versus Policy

Fearing rigid empirical criteria could discourage creativity and enthusiasm, Williams and Miller (1981) have suggested that criteria for publication should be different from those used in judgments of design quality. In their view, suitable criteria for publication are originality, applicability, and degree of clinical concern. It would be inappropriate to establish policy based on the current outcome research for IMT, but degrees of clinical concern, applicability and originality would make publication of such research worthwhile. The subsequent value of publishing is in the dialogue, debate and refinement of the issues as is aptly illustrated by the process seen in the reviews and rejoinders of Gurman and Kiskern, of Jacobson, and of Wells and Gianetti.

Principle versus Practice

Wells and Gianetti (1986) have recently criticized Gurman and Kniskern (1986) on grounds that their earlier conclusions about the questionable efficacy of IMT have caused marriage and family therapists to eliminate an individual mode of practice. Wells and Gianetti (1986) have inferred therefore that an exclusive reliance on a conjoint mode of practice penalizes clients who do not have an available or cooperative spouse. This is an unwarranted criticism. For better or for worse, marital therapy research has probably had but minimal impact on clinical practice. A clinician's decision-making process regarding format of treatment is more likely a function of theoretical orientation, clinical concerns/diagnosis, realistic limitations (time, money, spouse availability) or clinical skills than of delving into a research document.

Evaluative versus Scientific Research

In assessing the outcome research to date on IMT, it would seem most beneficial to perceive the research as evaluative and not scientific. In fact it would seem that the primary error made by Gurman and Kniskern (1978a, 1978b) was that of presenting the outcome research on IMT as scientific rather than evaluative. In logical sequence, Wells and Gianetti (1986) and Jacobson (1978) have critiqued the outcome research based on the category of scientific and have concluded that outcome research on nonbehavioral IMT (more generally for Jacobson, nonbehavioral marital therapy) is inadequate and inconclusive and therefore meaningless and useless. It is the position of the present author that outcome research on IMT, while inadequate and inconclusive from a scientific viewpoint, may nevertheless be meaningful and useful

from an evaluative viewpoint.

Smith, Glass and Miller (1980), and more recently Basham (1986), have concurred with the distinction made between evaluative and scientific research. Basham (1986) has stated:

> The crucial difference between a scientific and evaluative strategy is that the scientific strategy is oriented toward explaining how psychotherapy works in terms of theoretical mechanisms, whereas the evaluative goal is to assess the basic worth of psychotherapy by determining the effects it has on its recipients. (p. 91)

Scientific research is generated by a curious question in search of lawful relationships. The objective of scientific research is verification and conclusions under controlled conditions which will allow increasingly specific explanations. The derivative of explanation is facts, and with facts comes meaning and truth. Scientific research is the attempt to establish scientific truth (Smith, Glass & Miller, 1980; Basham, 1986).

In contrast to scientific research, evaluative research is generated by clinical concern in search of the resolution of a practical problem. The objective of evaluative research is the exploration of an issue in an applied setting plus the generation of further investigative questions. The derivative of this exploration and generation is a decision regarding clinical utility and application. Clinical utility and application is therefore the primary basis of meaning. Evaluative research is the attempt to assess the clinical utility of scientific truth (Smith, Glass & Miller, 1980; Basham, 1986).

Specific mention of the improvement and deterioration rates of IMT in the Gurman and Kniskern (1978a, 1978b) critical review of outcome research on nonbehavioral marital therapy addresses an area of clinical concern. It is not unusual for the marital therapist to be approached

by only one spouse seeking help for a distressed marriage. By empirical standards, the effectiveness or ineffectiveness of nonbehavioral IMT cannot be concluded, yet the exploration and generation of investigative questions on this issue should be seen as a valuable and worthwhile endeavor.

Summary

It is the position of the present author that the earlier appraisals by Gurman and Kniskern (1978a, 1978b) of the improvement and deterioration rates for nonbehavioral IMT were based on studies which do not meet minimum requirements for outcome research. To date, there is little acceptable empirical evidence either for or against the efficacy of IMT. This is a conclusion supported by Wells and Gianetti (1986), Jacobson (1978) and also recently accepted by Gurman and Kniskern (1986), and by Gurman, Kniskern & Pinsof, (in press).

While there is little acceptable empirical evidence for or against the efficacy of IMT, there is a substantial body of acceptable empirical evidence for the efficacy of conjoint marital therapy. Conjoint marital therapy remains the treatment of choice for marital problems.

Given the shortcomings of the empirical research on nonbehavioral IMT, the present author argues that the IMT research done to date is both valuable and worthwhile. Contrary to the position of Jacobson (1978) and of Wells and Gianetti (1986), who seriously question the value of studies with major flaws in research design and methodology, the present author affirms their value based on a clarification of the goals of research.

In addition, the debate described in this paper between Gurman and Kniskern, Jacobson and Wells and Gianetti has certainly clarified issues and suggested directions for further research.

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